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A NATURALIST'S YEAR.

By GRANT ALLEN.

XX.—WILD PEAS.

AMONG the hedgerows here, on the side of a deep Surrey lane, the lithe curling stems of the common English yellow pea clamber in their usual straggling fashion over the loose outlying sprays of bramble and hawthorn bushes. Looking close into the hedge, you can see that they festoon themselves by means of their tight-twisted tendrils, which wind and insinuate their graceful coils round and round the successive supports by whose aid they struggle gradually upward towards the air and light. Strange as it sounds to say so, these thin spiral tendrils are in reality metamorphosed leaves, or perhaps one ought rather to put it, the abortive footstalks of undeveloped leaflets. Pull off one of the whole leaves from the stem, and you will see it consists of a central midrib or common leaf-stalk, with two little arrow-headed stipules at its base, and one pair of very narrow leaflets half-way up; but the end of the leaf-stalk bears no more leaflets, and in their place it ends in a branched tendril, which really represents the altered and modified remnant of the remaining blades. See here, by the side of the yellow peas I have picked for comparison, two other leaves belonging to the same peaflower tribe; one is the pretty purple-tufted vetch (*Vicia cracca*), a common wayside plant over all Britain; the other is the bright golden Hippocrepis, abundant in chalky pastures and on limestone banks in Southern England. You will observe that the tufted vetch has several pairs of leaflets to each leaf, arranged in couples on opposite sides of the common stalk, instead of one pair only, as in the yellow pea; but at the end, it terminates in a branched tendril of much the same character as that of its yellow ally; while in the Hippocrepis each leaf-stalk, besides bearing some four or eight pairs of lateral leaflets, ends also in a single terminal leaflet, which occupies the place taken by the tendril both in the tufted vetch and in the yellow pea. These two cases help us to understand the line of development by which the leaflets at the end of the stalk in the most clambering species of peaflowers have been gradually metamorphosed into twining tendrils. If we take a rapid glance at the general type of foliage in the entire family, and at the special modifications which that type undergoes in adaptation to the varying environments of diverse divergent forms, we shall still better understand the nature and origin of these peculiar elongated leaf-organs.

The original and central peaflower leaf, from which all the other leaves of the group are derived by suppression or alteration of particular features, consists of a long central leaf-stalk, terminating in a single leaflet, and with other similar leaflets arranged in pairs down the opposite sides. This form of leaf is very well seen in Hippocrepis; and among other English plants of the family which exhibit it equally well, I may mention the pretty little bird's-foot (*Ornithopus perpusillus*), the common lady's-fingers (*Anthyllis vulneraria*), and the beautiful pale pink sainfoin (*Onobrychis sativa*), if not indigenous, at least now fully naturalised in the southern and eastern counties. That this long pinnate type of leaf is the original one we can gather both from the ease with which all the other forms can be derived from it, from their occasional relapse into it, and from the fact that it also appears in some of the simplest roses, of which the peaflowers are genetically a slightly specialised offshoot. This central type of leaf occurs mainly in those peaflowers which grow in open ground, where all the blade is freely exposed to air and sunlight.

The easiest variation upon the central type is found in certain divergent peaflowers, like the clovers, which intermingle freely with the grasses in meadows, and have to compete with them for their fair share of space and sunshine. In these cases the leaflets on the lower part of the leaf-stalk are never developed, because they could do no good to the plant; they would be over-shadowed by the grasses and other tall weeds on every side of them. Hence they would naturally grow smaller and smaller by disuse with each generation; while, at the same time, those plants which showed the greatest tendency to get rid of them, and to produce the three upper leaflets only (the terminal one, and a single pair below it), would be most highly favoured in the struggle for existence, because they wasted none of their material in places where it would be comparatively ineffective. So, in the long run, the clovers have come to possess the familiar trefoil type of leaf, consisting of three broad and expanded leaflets, all equally exposed to the sunshine at the top of their naked and elongated leaf-stalk. The common lotus shows us an intermediate stage between this type and the primitive pinnate form, for in that case there are five leaflets, three arranged in a trefoil at the end of the stalk, and two broad ones lower down, practically usurping the place and function of the original stipules. Other English peaflowers of the trefoil group are lucerne, nonsuch, and the other Medicagos, besides the rarer Melilotus and Trigonella. Broom is a bushy example of the same sort; but in it the two lateral leaflets are often wanting, and only the terminal one is developed. This last state, well adapted for shrubs and bushes, has become habitual in the English genistas, like petty whin; but some exotic genistas, cultivated in our conservatories, still retain the trefoil type of foliage. Furthest removed of all from the central type on this line of development is the common gorse, where the adult leaves are reduced to mere stiff, simple prickles; but even in this instance we have some faint memorial left of the earlier habit; for, as I have pointed out here already, the young seedling gorse plants have trefoil leaves, which only gradually merge as the shrub grows older, first into long, thin leafy, blades, and finally into sharp and rounded bristles.

On the opposite line of development, towards the peas and vetches, we get this system of suppression among the leaflets exactly reversed. The climbing types get rid, not of the lowest, but of the terminal leaflets; and for this very

good reason, that they want to use the ends of their leaf-stalks as tendrils to fasten them firmly on their unwilling hosts. The vetches are not quite such highly-developed plants as the true peas, and they supply us accordingly with some very interesting gradations in this direction. There is one rather uncommon British form (*Vicia orobus*), remarkable for its stem being more erect and its habit less climbing than in other vetches; and this form has a terminal leaflet to all the upper leaves, though in the lower ones the common leaf-stalk ends instead in a very rudimentary tendril, represented merely by a short point. In another slightly more advanced type (*Vicia tetrasperma*) there is always present a terminal tendril instead of the last leaflet; and sometimes this tendril is branched, that is to say in other words, a pair of lateral leaflets have also been reduced to naked twining leaf-stalks. In our other vetches, the tendrils are almost always branched, and one very developed type (*Vicia bithynica*), confined in Britain to the West Country, has only one pair of leaflets left. Of course, as these two have to do all the work, they have grown comparatively large and long, while in the other vetches, where the leaflets are many, they are usually small and broad. The true peas, forming the genus Lathyrus, show us a still further progress in the same direction. We have one very rare British species (it occurs with us only in Perthshire and Forfarshire, and is everywhere a dying mountain type) which resembles *Vicia orobus* in having a good many leaflets on each leaf, and in the occasional reappearance of the terminal leaflet in the upper foliage. But most of our peas have very few leaflets—indeed sometimes two pairs, and sometimes only one, all the rest being converted into tendrils to aid the climbing habit. At the same time the work of foliage is thrown a good deal on the stipules, which grow out accordingly into large, flat, leaf-like organs, far more conspicuous in the garden pea than the true leaflets, while the common leaf-stalk also shows a tendency to be winged or expanded at the side, which is very noticeable in the "everlasting pea" of our flower-beds and trellises. These two last tendencies find their final outcome in two English wild peas which have no real leaflets at all, having entirely substituted for them these auxiliary foliar organs. One is the so-called yellow vetchling (*Lathyrus aphaca*), a weed of cultivation which appears very occasionally in the cornfields of our southern counties; it has the two stipules immensely enlarged into what looks like a pair of big opposite heart-shaped leaves, with only a slender branching tendril between them to represent the original leaf-stalk and leaflets. The other is the grass-pea (*Lathyrus nissolia*) a pretty, grass-like plant, with beautiful pale-red flowers; it grows among tall grasses on the borders of fields, and has been compelled by its situation to imitate the shape of the surrounding blades, which it does by flattening out the common leaf-stalk into a long narrow ribbon, without stipules, leaflets, or even tendrils, save in the form of a fine point. The everlasting pea, and some south European forms, give us a good idea of the stages by which this curious transformation has been effected. The final result is that the grass-pea looks in foliage almost like a grass or sedge, and does not in any way recall its real ancestry from a highly compound pinnate leaf like that of sainfoin or Hippocrepis.

*** The picture of the Whirlpool Rapids promised for last number of Knowledge will appear next week. It presents the Rapids as shown in an instantaneous photograph, taken at a moment when the collision of the water particles, so graphically described by Professor Tyndall, had just taken place.

PLEASANT HOURS WITH THE MICROSCOPE.

By Henry J. Slack, F.C.S., F.R.M.S.

AMONG the objects mentioned in the last paper as affording beautiful specimens of plant-hairs strengthened by silex, was the cuticle of *Deutzia scabra* leaves. It is not possible to make good preparations of this cuticle by mechanical means, but it is easily effected by chemical agency, and a similar plan is useful in a variety of other cases. A perfectly sound well-grown leaf of *Deutzia scabra* should be cut into pieces about one-third of an inch square, and the thick mid-rib rejected. Pieces of this size will lie at the bottom of a large test-tube, six inches long and one inch wide. If the experimenter is not provided with any apparatus to hold such a tube, one can easily be made by sticking a piece of stout wire eight or nine inches long into a small square block of wood, and bending the top of the wire into a ring just wide enough to hold the tube by its projecting rim. The ring should be turned on one side, so as to keep the tube in a slanting position. This being arranged, pour some nitric acid into the test-tube, so as to cover the bits of leaf and occupy a space of about one inch in height. A spirit-lamp should be held under the tube, and the acid made quite hot. Small crystals of chlorate of potash should then be dropped in, one or two at a time. They excite a brisk effervescence. If added too quickly, or the acid is heated too highly, the mixture swells up suddenly, and may even boil over. This must be avoided, as a violent treatment of the leaf tears the cuticle into minute fragments, and from these no fine slides can be prepared. If the frothing is too slow, a little more heat should be applied, and the dropping in of the chlorate of potash continued until nothing is seen but a collection of white films floating on a bright yellow fluid. The tube must now be allowed to cool, and then with a little dexterity all the yellow fluid can be poured off, and the films allowed to remain. They must be washed several times by filling the tube with plain clear water and pouring it off again. All these things must be done gently, so as not to tear the films.

Those accustomed to chemical operations will not need more instruction, but for beginners further information is necessary. The process is one of oxidising, or burning away, all but the leaf cuticle. The oxygen is applied at the expense of the chlorate of potash and of the nitric acid, and as soon as the action goes on briskly, the latter gives forth orange fumes of nitric-oxide gas.* This is not only very unpleasant, but extremely unwholesome to breathe. A good mouthful of it, even when largely mixed with common air, is very suffocating and highly injurious to the lungs. The fumes should be allowed to go up a chimney or out of window. The operator should notice which way the air-currents carry them, and keep, as sailors say, to the leeward. Beginners should make their first experiment on a smaller scale than has been mentioned. Nitric acid burns holes in clothes and stains the skin yellow. Having completed the process as described, the films may be kept in a small bottle, with a little water and a few drops of alcohol. To prepare a slide, take up a few films on a needle and drop them into a small shallow saucer of clear water; or, if a very small quantity is on the needle, it may be placed on the centre of a glass slide in a water-drop. The first plan is the best, as most certain to remove any chlorate of potash that may not have

* When nitric oxide meets common air, it instantly seizes its oxygen and becomes the orange gas nitrous acid, which is readily absorbed by water and is highly corrosive.

been washed out in the former processes. If the films have lost all their air-bubbles they may not float, but stirring the water will make them do it, and a few small pieces can be fished out with a thin spoon or a knife-blade, and dropped on to a glass slide. They must then be spread out very carefully with fine needles stuck in wooden handles, or a fine sable pencil such as miniature painters use. The object is to get some films spread out quite flat. They will most likely be found folded up, or one over another. The least force tears them, but under a hand-magnifier or a dissecting microscope success is easily obtained. Having arranged a few films nicely in a water-drop, the slide should be allowed to dry slowly where no dust can get at it. When it is quite dry, put a drop of Canada balsam, thinned with benzine, on to a glass cover, and press it gently on to the films. The cover may be held with a spring clip, such as opticians sell for the purpose, and the balsam must be allowed to harden before the slide is fit for use. A hint may be given about the Canada balsam. It is much too thick for most purposes in the state in which it is usually sold. Pure benzine, or benzole, as it is also called, dissolves it readily, so much so that it is very useful in cleaning the superfluous balsam off slides. When used for thinning, it must be added cautiously, and the balsam bottle put in a warm place. When it is about as thick as golden syrup treacle—not quite thin enough for the films—further additions of benzine act rather suddenly, and the thinning may be carried too far. If so, the extra supply can be evaporated; but it is well to hit the right quantity.

Amongst the objects which yield beautiful results with the acid and chlorate of potash treatment are the needle-shaped leaves of the pine-trees. *Pinus Austriaca*, common in shrubberies, is a good one for the purpose. Quite clean leaves should be selected, of fresh growth. They should be cut into short lengths, so as not to require much acid to cover them, and treated exactly as the Deutzia leaves, but they want a little more cooking. When finished they are quite white, and in the state of hollow tubes, all their insides being eaten out. To prepare for the microscope, a piece of the tube must be slit open and flattened out on a slide with fine needles in a drop of water. If it curls up it must be flattened again and kept so by a covering-glass. When quite dry, mount in balsam, and view with a half-inch objective polarised light and a selenite film. A hand-magnifier is sufficient to show that fir needles are ornamented with rows of white glistening spots. In these the stomata of the plant are situated. Their action upon polarised light is very beautiful, and the changes obtainable by rotating the prisms very striking. Very elegant patterns that would be popular for ladies' dresses, window-curtains, &c., readily appear. So far as the writer knows, these pine needles have been generally neglected by microscopists.

TRICYCLES IN 1883.

SMALL WHEELS *v.* LARGE WHEELS.

By John Browning,

Chairman of the London Tricycle Club.

CONSIDERATIONS of space prevented me from exhausting this subject in my last article, though I had left several points of great importance untouched on.

The weight of machines with large wheels is never felt so injuriously as when pushing up hill. It has been roughly calculated that out of each ten miles of road in England one mile, at least, will be a hill too steep to be rideable.

Let any person take a ride of, say, even fifty miles in a day on a tricycle with small wheels, weighing 60 lb., over a moderately hilly road, and then repeat the ride over the same course on a machine with, say, 52-in. wheels, weighing 96 lb. to 98 lb., which is a common weight for such a machine. After pushing this machine up the hills, I will warrant he will have had enough of large wheels.

Last year I rode a "Premier Sociable," with 48-in. wheels. As I wished to drive the machine throughout the day, if I pleased, without my wife once putting her feet on the pedals, I had it geared down to 36 in. The weight of this machine was 172 lb. My pace, when riding with my wife, was about four miles an hour, and our journeys averaged twenty miles a day.

This year I have been riding an "Apollo Sociable" I had specially made for me, with 44-inch wheels geared down to 36 inches, as in the former case, but the weight of this machine is only 133 lb. Our pace on this is improved to five miles an hour, and we average about 30 miles a day with ease, and have ridden 7½ miles within forty minutes, and 23 miles in four hours. These we did easily, though I pushed the machine up more than three miles of hills alone. With the additional experience I have now gained I consider I should have done better by having the "Apollo" made with 36-inch wheels geared level.

I thoroughly disagree with all gearing-down. When a low gearing is required, let the wheels be made small instead, and the rider will have a lighter, stronger, and more compact machine.

Weight is not everything, I admit; but tricyclists must make it the first consideration. It is the principal cause of the exhaustion often complained of by riders. When I rode a single machine which weighed 109 lb., I could only ride fifteen miles without requiring a meal. Now I have a machine which weighs just over 50 lb., I commonly ride twenty-five miles between meals, and I have ridden twenty-nine miles without refreshment.

Regarded as performances these examples are, of course, very poor, but being born almost without muscles, I am compelled to ride with my brains, and my illustrations are none the less useful on that account.

Another point greatly in favour of small wheels is that they offer scarcely any resistance to wind compared to large wheels. I have frequently seen men almost pulled up by wind on machines with 50-in. wheels, while others, who were not stronger riders, were travelling without much difficulty on machines with 40-in. wheels.

The truth is, that I have been advocating the use of small wheels for years with a definite object in view. I wished to see our machines combine the ease of the bicycle with the comfort and safety of a tricycle. To accomplish this, I saw that we must obtain a great reduction in their weight. We cannot dispense with the weight of the third wheel and its friction on the road, but we need have no more. When I began my experiments, tricycles weighed about twice as much as bicycles. I soon found that the greater part of the weight was in the two driving-wheels. It was, therefore, absolutely necessary to reduce the size of these if we wished to make any considerable reduction in the weight of the machine. This can be done without any disadvantage of consequence, because the tricycle, unlike the bicycle, admits of being geared up so that the pedals need not make a greater number of revolutions with the smaller wheels.

The driving-wheels must, of course, revolve oftener—let them do so; with ball-bearings the small additional friction may be neglected.

Some strong riders, I know, object to fast pedalling, and prefer to work with greater pressure on their pedals. This

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is no argument against small wheels; on the contrary, I consider it is one of the strongest points in favour of them, that the rider may have them geared up to any degree that suits him, still keeping his machine as low in weight as possible.

There have been complaints, I am aware, of machines with small wheels giving a painful amount of vibration; but I have in every such case traced it to another source—namely, that with small wheels the makers have given very small tyres, in the endeavour to make the machine lighter with as little trouble to themselves as possible.

Now, the smaller the wheels and the lighter the machine, the larger the tyres require to be to lessen the vibration, both to the machine and the rider. Small wheels must have tyres at least the same size as are usually put on large wheels, or they will be unfairly tested.

Only one objection that I know of can be urged against small wheels. Some persons dislike their appearance. It is difficult to argue on matters of taste; but I think small wheels look best, because with large wheels the machine predominates over the rider, while with small wheels the man dominates the machine.

THE AMATEUR ELECTRICIAN.

BATTERIES.—IV.

IT was observed in the preceding article that the Leclanché cell introduced a great peculiarity in the matter of the negative element and its surroundings. The utility of the battery is so great that several modifications in its construction have been devised. One of them is the "Gravity" Leclanché. It is generally made in troughs of ten cells, each cell being separated from its neighbours by means of slate or other suitable material coated with

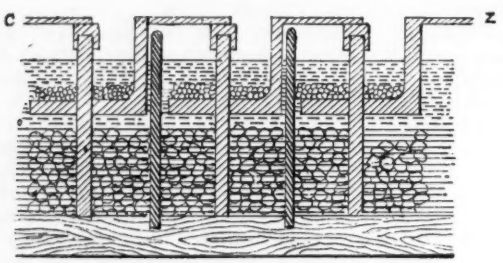

Fig. 1.

marine glue.* Fig. 1 is an illustration of the battery. The zinc plate is of a peculiar shape (as shown in Fig. 2), and is supported by the strap resting on the slate partition. The carbon is in the form of a rectangular rod or block, which, passing through the hole in the zinc plate, dips into a quantity of broken carbon and manganic dioxide. Care must be taken that the carbon black does not touch the zinc. A layer of sal-ammoniac is placed on the zinc plate, and water being added, it is gradually dissolved. A copper strap cast into the plate, and serves to connect it to the carbon in the next cell.

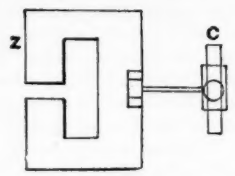

Fig. 2.

The action is, naturally, the same as in the ordinary Leclanché, the advantage gained being a considerable reduction in the resistance consequent on the omission of the porous earthenware pot. The resistance is 1·5 Ohms, the E M F being of course the same as in the ordinary form. Care should be taken not to disturb the battery, nor should it be forgotten that it is advisable, if not absolutely necessary, to leave the lid of the trough (should it have one) slightly raised, to permit any free ammonia that may be given off to escape. As is the case with all batteries, cleanliness is essential. The salt (sal ammoniac) has a strong tendency to crystallise, but a damp cloth passed over the edges of the cells occasionally will suffice to prevent this.

Another modification, having for its object the omission of the porous pot, but free from the difficulties attending the use of the gravity form, is that known as the "Agglomerate." This, too, has several varieties. It will be sufficient if we confine ourselves to two of them. In the first, the negative element consists of a plate or flat block of carbon placed between two blocks of compressed manganic peroxide. India-rubber bands keep the three blocks in contact, the porous pot being thereby dispensed with.

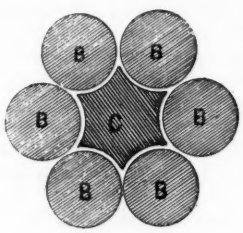

Fig. 3.

The form which has met with greater favour is, however, of a somewhat different shape. Fig. 3 is a horizontal section of the negative element which consists of a fluted block of carbon (*c*), having a circular rod of compressed manganic peroxide (B) in each of the six flutes. A piece of coarse canvas passed round and secured by a couple of elastic bands keeps the parts together. In the previously-described agglomerate cell the ordinary zinc rod is generally used, but in this one zinc cylinders are used, the result being a very considerable reduction in resistance and greater constancy on circuits of high resistance. The resistance is but 0·1 of an Ohm, while as a demonstration of its greater constancy it may be mentioned that a battery of forty agglomerate cells has for a long time past been employed on one of the busiest London and Liverpool telegraph wires. It is also worthy of notice that no less than 2,000 of these cells are at present in use at the

* Marine glue is a substance insoluble in water and acids, but readily soluble in carbonic disulphide (CS_2). Originally it was composed of caoutchouc dissolved in naphtha and shellac, but the cost of the ingredients is too high for general purposes, and the material now known as marine glue is made from less costly constituents. It acquires a treacley subsistency on gently heating, and cools very rapidly. For such purposes as it is usually employed by amateurs it may be worked with little difficulty. It may be heated in an old iron ladle over a gentle fire, and then poured out as required; or small pieces of it may be laid on the substance to be coated, and an old file or other piece of iron, heated to a dull red, passed over it, but not allowed to touch it until the heat is considerably reduced. An even layer may be very easily made in this way. Some complain that they cannot work the glue, but where this is the case it can only be through downright clumsiness.

Central Telegraph Office in London. Were the resistance of the circuit low, polarization would speedily ensue. Nevertheless, the agglomerate cell is to be preferred to the ordinary Leclanché, even for amateur purposes. The blocks are very durable, and after nearly two years' hard service show little or no signs of giving way.

It is still an open question whether the agglomerate cells have any advantage over the parent form, other than is derived from the absence of the porous pot and the increased zinc surface. Consequently an excellent form is that known as the Grénet, in which the porous pot is replaced by a stitched canvas bag to hold the manganic peroxide and carbon. In this instance all the gain pertaining to the agglomerate is embraced.

The Bennett cell is in principle somewhat akin to the Leclanché, but for general work is not to be compared with it. The positive element is a piece of zinc immersed in a solution of caustic soda, a compound which has the merit of being not only inexpensive but equally nasty. A porous cell containing the soda solution is stood in a tin, or rather tinned-iron, can, the space between the can and the porous pot being filled with iron turnings. The solution gradually penetrates the porous pot, and, damping the turnings, a circuit is formed and a current generated. This current is, however, very spasmodic, and even less constant than that from the Leclanché. It can, therefore, found no great claim on its efficiency, while on the other hand it enjoys the privilege pertaining to all batteries containing iron, of perfuming the air with sulphuretted hydrogen—an odour which, if it is like anything on this earth, resembles most strongly the delicious aroma of rotten eggs. When the cell was first introduced, accompanied as it was with loud and general acclamations, it was recommended as being extremely cheap in construction, the outer or containing vessel being an old preserved meat-can. We noticed, however, that those sent out by the company which enjoyed the privilege of selling it, were contained in specially made tins, having a closely-fitting cover, presumably to bottle up the scent. It is manifest from what we have said concerning the cell that it is impossible for us to recommend its adoption for any purpose whatever.

We shall next turn our attention to a large class of batteries, embraced under the generic appellation of "Bichromate," and amongst them we hope to see some of exceptional utility to the student.

LAWS OF BRIGHTNESS.

IX.

By Richard A. Proctor.

IT remains only that I should consider the effect of instrumental appliances for increasing the degree of light which we receive from objects.

The telescope is an appliance of this sort; in fact, we may regard as the main feature of the telescope its power as a light-gatherer. In whatever proportion the object-glass of a refractor, or the mirror of a reflector, exceeds in area the pupil of the eye, in the same proportion (neglecting loss of light by imperfect reflection, or through refraction) may the quantity of light received by the eye when the telescope is used exceed the quantity received from the same object when the eye is unaided. I say *may* rather than *does;* for matters may be so arranged that the full light-gathering power of the telescope is not employed.

Now, it seems at first sight that in consequence of this relation the apparent brightness of an object observed with a large telescope must be enormously increased. And, as I mentioned in the first paper of this series, the idea seems monstrous to many that there cannot under any circumstances be an increase of apparent brightness of an object, though there may be an enormous increase in the total quantity of light received from the object. "Do you mean to tell me," an esteemed friend of mine once remarked, "that the moon is no brighter with my 6½ in. reflector than with the naked eye? It *must* be brighter. I can scarcely bear to look at the moon, it is so bright, when I use a low power and the moon is high up in a clear sky; but I have never found any trouble in looking at the moon with the naked eye." Be it understood that there was no question as to the point under discussion. It was the intrinsic lustre of the moon's surface, not the total quantity of light received from her, that we were both considering. I pointed out, though I am bound to admit the argument was not found to be convincing, that there is the same intrinsic degree of pain when one tooth is pulled out as if a dozen were extracted at once, yet a man might easily bear one operation who would faint under the other.*

But as a matter of fact the case of the telescope is exceedingly simple. Take, for convenience, the simplest form of the astronomical telescope, which gives the brightest images for a given magnifying power. Let O O′ (Fig. 14) be the object-glass, *e e′* the eye-glass; and let a pencil filling the whole object-glass converge to a focus at F, and after falling on the part *n n′* of the eye-glass emerge with parallel rays and fall on the pupil of the eye E. Its cross-section is either just equal in diameter to the pupil of the eye, or less, or greater. First suppose it equal. Then the total quantity of light received from the point of the image, exceeds the quantity which would fall on the pupil of the naked eye as $(O\,O')^2$ exceeds $(n\,n')^2$. But the magnifying power of the telescope is represented by the ratio

$$\frac{O\,F}{F\,n}, \quad \text{or, which is the same ratio,} \quad \frac{O\,O'}{n\,n'}$$

Therefore the apparent area of the object is increased in the ratio $(O\,O')^2$ to $(n\,n')^2$—the precise ratio in which the total quantity of light received from the object is increased. This increase, then, is just competent to make the increased area as bright as the area seen by the unaided eye.

Fig. 14.

But next suppose *n n′* less than the diameter of the pupil, and call this diameter *d*. Then, as the whole of the emergent pencil falls on the pupil, the increase of the total quantity of light is as $O\,O'^2$ to d^2, but the magnifying power of the telescope is, as before, $\frac{O\,O'}{n\,n'}$, and the magnification of areas is as $O\,O'^2$ to $(n\,n')^2$. Thus the magnification of areas is to the total increase of light as

$$\frac{(O\,O')^2}{(n\,n')^2} : \frac{(O\,O')^2}{d^2}$$

or as

$$d^2 : (n\,n')^2.$$

But *d* is greater than *n n′*; hence the apparent area of

* I suppose this to be the case, but do not speak from experience. I have heard, however, that even a man of the strongest frame would be shaken if a dozen teeth were extracted in quick succession; and I presume the extraction of a dozen at the same instant would be even more trying. By extraction, I mean fair deliberate lugging out, such as the soul of the dentist loveth—not knocking out by a blow; though the knocking out of a dozen teeth at once can hardly be a pleasant experience.

XUM

the object is increased in a greater proportion than the light. Therefore the apparent brightness of the object is diminished.

Lastly, let us suppose $n\,n'$ greater than d. Then the magnification of areas is, as before, represented by the ratio $\frac{(O\,O')^2}{(n\,n')^2}$; and the increase in the quantity of the light falling on the eye is as $(O\,O')^2$ to d^2; but this quantity does not enter the pupil, because it comes out in a pencil of parallel rays having a cross-section of diameter $n\,n'$, and the pupil has only a cross-section of diameter d; the proportion entering the eye is thus reduced in the proportion of the areas of these cross-sections, or as d^2 to $(n\,n')^2$. Hence the quantity of light actually received by the pupil is represented by the proportion

$$\frac{(O\,O')^2}{d^2} \cdot \frac{d^2}{(n\,n')^2}, \text{ or } \frac{(O\,O')^2}{(n\,n')^2},$$

which is the proportion in which the apparent area of the object is increased. Hence the brightness remains unchanged, as in the first case. The only difference, in fact, between this case and the first is that the telescope is not in the last case pushed to the full power it will bear without a reduction in the apparent brightness of objects: in fact, only a portion of the object-glass is really used in bringing light to the eye from any point of the object. Light from each point of an object, in such a case as this, employs (as it were) its own part (a circular part) of the object-glass.*

Of course, the above reasoning does not apply in the case of a star, because a star is *not* apparently magnified by a telescope; but on the contrary, owing to the operation of laws with which we are not at present concerned, the image is reduced. Thus, the brightness of a star is increased pretty nearly in proportion to the area of the object-glass or mirror.

But even in the case of stars the law above considered has its analogue. Take an imaginary case, where (say) twenty stars form a cluster (each star being visible to the naked eye) and the telescope shows those twenty stars and no more; then, the apparent brightness of the cluster, regarded as a whole, remains unaltered when the telescope is used; for in precise proportion to the increase in the brightness of the individual stars is the increase in the apparent area of the cluster.†

The application to the planets, moon, sun, &c., is obvious.

It may be asked why night-glasses show objects which the unaided eye cannot see; and again why, by the use of single magnifying glasses we can read at dusk what would be undiscernible, or only discerned with difficulty, by the naked eye. The answer simply is that the quantity of light received from the object is increased. It is easy to test this.

Let the observer look at the page of a book with a magnifying glass in the dusk of evening, and notice whether the part seen with the glass (and rendered legible by its means) seems a brighter circular spot on the darker ground of the rest of the page, or *vice versâ*. If he is careful not to let the diffused light from the glass reach his eye, he will see at once that the legible part of the page looks darker than the part outside, which nevertheless is illegible.

It is equally clear, but the point requires to be more carefully dwelt upon, that we cannot by increasing the size of telescopes increase the apparent brightness of such objects as nebulæ, comets, the zodiacal light, &c. In the case of very large objects like the zodiacal light we cannot even increase the total quantity of light received by the eye. In the case of comets, nebulæ, &c., we can do so; and so we can render objects of this sort discernible, or even conspicuous, which otherwise would remain unseen. But it is important to notice that we cannot increase their brightness. It is this which constitutes one of the great difficulties in applying the spectroscopic analysis of these objects; for in such analysis it is often convenient to use a rather narrow slit, and therefore the spectroscopist cannot avail himself of the great quantity of light gathered up for him by his telescope. Some astronomers, therefore, in examining the nebulæ which give three or four bright-line spectra, use an open slit, or no slit at all, getting three or four *images* of the nebula, instead of three or four lines.

In another kind of research—the study, namely, of those faint but widely extending nebulosities which Sir W. Herschel was the first to notice—a large telescope is of no great use. I am persuaded, indeed, that the naked eye can do more in work of this sort than is commonly supposed, care being taken to protect the eye from extraneous light, and also to conceal the lucid stars from view.

Autumn Leaves.—Messrs. Sampson Low, Marston, & Co. are issuing for the autumn holidays a new edition of Mr. F. G. Heath's "Autumnal Leaves"—a work which the author claims to be the only one ever published in Europe or America giving actual facsimile representations in colour and venation of autumn-tinted foliage.

A Carbonaceous Meteorite.—An interesting meteorite which fell in the province of Entre-Rios, Argentine Republic, has been examined by M. Daubrée. The meteorite recalls certain kinds of lignite and clay coals, such as the boghead coal. In the black paste of the meteorite are to be seen small angular grains of a bottle-green of a hyaline colour, others again are whitish. The whole appearance of the stone resembles some of our volcanic conglomerates. Brass-yellow grains like pyrrhotine are also visible, and reddish spots like the chloride of iron sometimes seen in meteorites. Chemical analysis shows that the meteorite contains iron, lime, and magnesia. The most important peculiarity of the meteorite is, however, that it contains carbon in an organic form. This is chiefly proved by the action of potash on it. In fact, Dr. Daubrée hopes yet to find a meteorite bearing organic remains. Other carbonaceous meteorites have fallen in divers places, but none, so far as we are aware, have yielded traces of organic life.—*Engineering.*

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* The case corresponds to that of an opera-glass (for it is easily seen that the reasoning would not be affected by substituting a Galilean for a simple astronomical telescope). Accordingly it will be found that in using an opera-glass we can hide or dim a portion of the field of view by covering a part of the object-glass. But in the case of a telescope used (as the astronomical nearly always is) with its full power, covering a part of the object-glass dims the whole field equally. Dr. Huggins told me, six or seven years ago, of an old telescope he bought, in which the object-glass was four or five inches in diameter, and the focal length little more than a foot. The emergent pencil was an inch or so in diameter (with the power actually employed). As Dr. Huggins quaintly remarked, it was "a telescope for a horse, not for a man."

† In passing, I may notice that the same law applies to the effects of distance. A cluster of stars, separately discernible, would remain just as bright regarded as a whole (that is, not considering the total quantity of light received from it, but its brightness as related to its area), however far it might be removed, so long as the separate stars continued discernible. Nor would it grow any brighter, as a whole, however near it might approach to the observer. (It is assumed here for the moment, that there is no extinction of light in the interstellar spaces.)

THE CHEMISTRY OF COOKERY.

XVII.

By W. Mattieu Williams.

FRYING IN OIL.

EGARDING the fat used in frying as a medium for conveying heat, freedom from any special flavour of its own is a primary desideratum. Olive oil of the best quality is almost absolutely tasteless, and having as high a boiling point as animal fats it is the best of all frying media. In this country there is a prejudice against the use of such oil. I have noticed at some of those humble, but most useful establishments where poor people are supplied with penny or twopenny portions of good fish, better cooked than in the majority of "eligible villa residences" that in the front is an inscription stating "only the best beef-dripping is used in this establishment." This means a repudiation of oil. Such oil as has been supplied for fish-frying may well be repudiated.

On my first visit to Arctic Norway I arrived before the garnering and exportation of the spring cod harvest was completed. The packet stopped at a score or so of stations on the Lofodens and the mainland. Foggy weather was no impediment, as an experienced pilot free from catarrh could steer direct to the harbour by "following his nose." Huge cauldrons stood by the shore in which were stewing the last batches of the livers of cod fish caught a month before and exposed in the meantime to the continuous arctic sunshine. Their condition must be imagined, as I abstain from description of details. The business then proceeding was the extraction of the oil from these livers. It is, of course, "cod liver oil," but is known commercially as "fish oil," or "cod oil." That which is sold by our druggists as cod liver oil is described in Norway as "medicine oil," and though prepared from the same raw material, is extracted in a different manner. Only fresh livers are used for this, and the best quality, the "cold-drawn" oil, is obtained by pressing the livers without stewing. Those who are unfortunately familiar with this carefully prepared, highly refined, product, know that the fishy flavour clings to it so pertinaciously that all attempts to completely remove it without decomposing the oil have failed. This being the case, it is easily understood that the fish oil stewed so crudely out of the putrid or semi-putrid livers must be nauseous indeed. I am told that it has nevertheless been used by some of the fish fryers, and I know that refuse "Gallipoli" (olive oil of the worst quality) is sold for this purpose. The oil obtained in the course of salting sardines, herrings, &c., has also been used.

Such being the case, it is not surprising that the use of oil for frying should, like the oil itself, be in bad odour.

I dwell upon this because we are probably on what, if a fine writer, I should call the "eve of a great revolution" in respect to frying media.

Two new materials, pure, tasteless, and so cheap as to be capable of pushing pig-fat (lard) out of the market, have recently been introduced. These are cotton-seed oil and poppy-seed oil. The first has been for some time in the market offered for sale under various fictitious names, which I will not reveal, as I refuse to become a medium for the advertisement of anything—however good in itself—that is sold under false pretences. If the lamp of Knowledge, more fortunate than that of Diogenes, should light upon some honest men who will retail cotton-seed oil as cotton-seed oil, I shall gladly (with the Editor's permission) do a little straightforward touting for them, as they will be public benefactors, greatly aiding the present movement for the extension of the use of fish food.

As every bale of cotton yields half a ton of seed, and every ton of seed may be made to yield 28 lb. to 32 lb. of crude oil, the available quantity is very great. At present only a small quantity is made, the surplus seed being used as manure. Its fertilising value would not be diminished by removing the oil, which is only a hydro-carbon, *i.e.*, material supplied by air and water. All the fertilising constituents of the seed are left behind in the oil-cake from which the oil has been pressed.

Hitherto cotton-seed oil has fallen among thieves. It is used as an adulterant of olive oil; sardines and pilchards are packed in it. The sardine trade has declined lately, some say from deficient supplies of the fish. I suspect that there has been a decline in the demand due to the substitution of this oil for that of the olive. Many people who formerly enjoyed sardines no longer care for them, and they do not know why. The substitution of cotton-seed oil explains this in most cases. It is not rancid, has no decided flavour, but still is unpleasant when eaten raw, as with salads or sardines. It has a flat, cold character, and an aftertaste that is faintly suggestive of castor oil; but faint as it is, it interferes with the demand for a purely luxurious article of food. This delicate defect is quite inappreciable in the results of its use as a frying medium. The very best lard or ordinary kitchen butter, eaten cold, has more objectionable flavour than refined cotton-seed oil.

I have not tasted poppy-seed oil, but am told that it is similar to that from the cotton-seed. As regards the quantities available, some idea may be formed by plucking a ripe head from a garden poppy and shaking out the little round seeds through the windows on the top. Those who have not tried this will be astonished at the numbers produced by each flower. As poppies are largely cultivated for the production of opium, and the yield of the drug itself by each plant is very small, the supplies of oil may be considerable; 571,542 cwt. of seeds were exported from India last year, of which 346,031 cwt. went to France.

Palm oil, though at present practically unknown in the kitchen, may easily become an esteemed material for the frying kettle (I say "kettle," as the ordinary English frying pan is only fit for the cooking of such things as barley bannocks, pancakes, fladbrod, or oat-cakes). At present, the familiar uses of palm-oil in candle-making and for railway grease will cause my suggestion to shock the nerves of many delicate people, but these should remember that before palm-oil was imported at all, the material from which candles and soap were made, and by which cart-wheels and heavy machinery were greased, was tallow—*i.e.*, the fat of mutton and beef. The reason why our grandmothers did not use candles when short of dripping or suet was that the mutton fat constituting the candle was impure, so are the yellow candles and yellow grease in the axle-boxes of the railway carriages. This vegetable fat is quite as inoffensive in itself, quite as wholesome, and—sentimentally regarded—less objectionable, than the fat obtained from the carcase of a slaughtered animal.

When common-sense and true sentiment supplant mere unreasoning prejudice, vegetable oils and vegetable fats will largely supplant those of animal origin in every element of our dietary. We are but just beginning to understand them. Chevreul, who was the first to teach us the chemistry of fats, is still living, and we are only learning how to make butter (not "inferior Dorset," but "choice Normandy") without the aid of dairy produce. There is, therefore, good reason for anticipating that the inexhaustible supplies of oil obtainable from the vegetable world—especially from tropical vegetation—will ere long

be freely available for kitchen uses, and the now popular product of the Chicago hog factories will be altogether banished therefrom, and used only for greasing cart-wheels and other machinery.

As a practical conclusion of this part of my subject, I will quote from this month's number of *The Oil Trade Review* the current wholesale prices of some of the oils possibly available for frying purposes. *Olive oil*, from £43 to £90 per tun of 252 gallons; *Cod oil* £36 per tun; *Sardine or train* (*i.e.* the oil that drains from pilchards, herrings, sardines, &c., when salted) £27. 10s. to £28 per tun. *Cocoanut* from £35 to £38 per ton of 20 cwt. (This in the case of oil is nearly the same as the measured tun.) *Palm* from £38 to £40. 10s. per ton; *Palm-nut or copra*, £31. 10s. per ton; *Refined cotton seed*, £30. 10s. to £31 per ton; *Lard*, £53 to £55 per ton. The above are the extreme ranges of each class. I have not copied the technical names and prices of the intermediate varieties. One penny per lb. is = £9. 6s. 8d. per ton, or in round numbers, £1 per ton may be reckoned as 1-9th of a penny per lb. Thus the present price of best refined cotton-seed oil is 3½d. per lb.; of cocoanut oil, 3¾d.; palm-oil, from 3⅓d. to 4½d., while lard costs 6d. per lb. wholesale—usually 7d.

I should add in reference to the seed-oils, that there is a possible objection to their use as frying media. Oils extracted from seeds contain more or less of *linoleine* (so-named from its abundance in linseed oil), which, when exposed to the air, combines with oxygen, swells and dries. If the oil from cotton-seed or poppy-seed contains too much of this, it will thicken inconveniently when kept for a length of time exposed to the air. Palm-oil is practically free from it, but I am doubtful respecting palm-nut-oil, as most of the nut-oils are "driers."

EVOLUTION OF HUMAN PHYSIOGNOMY.*

By E. D. Cope.

THE ability to read character in the form of the human face and figure, is a gift possessed by comparatively few persons, although most people interpret, more or less correctly, the salient points of human expression. The transient appearances of the face reveal temporary phases of feeling which are common to all men; but the constant qualities of the mind should be expressed, if at all, in the permanent forms of the executive instrument of the mind, the body. To detect the peculiarities of the mind by external marks, has been the aim of the physiognomist of all times; but it is only in the light of modern evolutionary science that much progress in this direction can be made. The mind, as a function of part of the body, partakes of its perfections and its defects, and exhibits parallel types of development. Every peculiarity of the body has probably some corresponding significance in the mind; and the causes of the former, are the remoter causes of the latter. Hence, before a true physiognomy can be attempted, the origin of the features of the face and general form must be known. Not that a perfect physiognomy will ever be possible. A mental constitution so complex as that of man cannot be expected to exhibit more than its leading features in the body; but these include, after all, most of what it is important for us to be able to read, from a practical point of view.

* Abstract of a lecture delivered before the Franklin Institute of Philadelphia, Jan. 20, 1881, in exposition of principles laid down in The Hypotheses of Evolution, New Haven, 1870, p. 31.

The present essay will consider the probable origin of the structural points which constitute the permanent expression. These may be divided into three heads, viz.: (1) Those of the general form or figure; (2) Those of the surface or integument of the body with its appendages; and (3) Those of the forms of the head and face. The points to be considered under each of these heads are the following:—

I.—The General Form.

1. The size of the head.
2. The squareness or slope of the shoulders.
3. The length of the arms.
4. The constriction of the waist.
5. The width of the hips.
6. The length of the leg, principally of the thigh.
7. The sizes of the hands and feet,
8. The relative sizes of the muscles.

II.—The Surfaces.

9. The structure of the hair (whether curled or not).
10. The length and position of the hair.
11. The size and shape of the nails.
12. The smoothness of the skin.
13. The colour of the skin, hair, and irides.

III.—The Head and Face.

14. The relative size of the cerebral to the facial regions.
15. The prominence of the forehead.
16. The prominence of the superciliary (eyebrow) ridges.
17. The prominence of the alveolar borders (jaws).
18. The prominence and width of the chin.
19. The relation of length to width of skull.
20. The prominence of the malar (cheek) bones.
21. The form of the nose.
22. The relative size of the orbits and eyes.
23. The size of the mouth and lips.

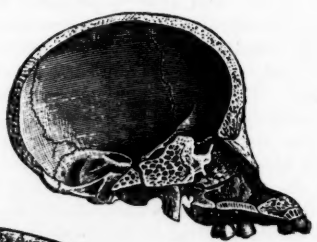

Fig. 2.

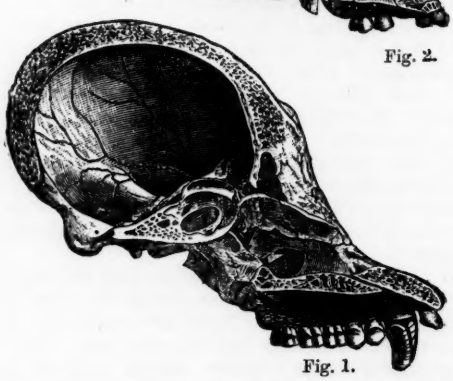

Fig. 1.

Fig. 1.—Section of skull of adult orang-outang (*Simia Satyrus*).
Fig. 2.—Section of skull of young orang, showing relatively shorter jaws and more prominent cerebral region.

The significance of these, as of the more important structural characters of man and the lower animals, must be considered from two standpoints, the palæontological and the embryological. The immediate palæontological history

of man is unknown, but may be easily inferred from the characteristics displayed by his nearest relatives of the order Quadrumana. If we compare these animals with man, we find the following general differences. The numbers correspond to those of the list above given.

I. As to General Form.—(3) In the apes the arms are longer; (8) the extensor muscles of the leg are smaller.

II. As to Surface.—(9) The body is covered with hair which is not crisp or woolly; (10) the hair of the head is short; (13) the colour of the skin, &c., is dark.

III. As to Head and Face.—(14) The facial region of the skull is large as compared with the cerebral; (15) the forehead is not prominent, and is generally retreating; (16) the superciliary ridges are more prominent; (17) the edges of the jaws are more prominent; (18) the chin is less prominent; (20) the cheek bones are more prominent; (21) the nose is without bridge, and with short and flat cartilages; (22) the orbits and eyes are smaller (except in Nyctipithecus); (24) the mouth is small and the lips are thin.

It is evident that the possession of any one of the above characteristics by a man approximates him more to the monkeys, so far as it goes. He retains features which

Fig. 3.—Portrait of a girl at five years of age.

have been obliterated in other persons in the process of evolution.

In considering the physiognomy of man from an embryological standpoint, we must consider the peculiarities of the infant at birth. The numbers of the following list correspond with those already used.

I. As to the General Form.—(1) The head of the infant is relatively much larger than in the adult; (3) the arms are relatively longer; (4) there is no waist; (6) the leg, and especially the thigh, are much shorter.

II. As to the Surfaces.—(10) The body is covered with fine hair, and that of the head is short.

III. The Head and Face.—(14) The cerebral part of the skull greatly predominates over the facial; (16) the superciliary ridges are not developed; (17) the alveolar borders are not prominent; (20) the malar bones are not prominent; (21) the nose is without bridge and the cartilages are flat and generally short; (22) the eyes are larger.

It is evident that persons who present any of the characters cited in the above list are more infantile or embryonic in those respects than are others; and that those who lack them have left them behind in reaching mat rity.

We have now two sets of characters in which men may differ from each other. In the one set the characters are those of monkeys, in the other they are those of infants. Let us see whether there be any identities in the two lists, *i.e.*, whether there be any of the monkey-like characters which are also infantile. We find the following to be such:

I. As to General Form.—(3) The arms are longer.

II. Surface.—(10) The hair of the head is short, and the hair on the body is more distributed.

III. As to Head and Face.—(21) The nose is without bridge and the cartilages are short and flat.

Three characters only out of twenty-three. On the other hand, the following characters of monkey-like significance are the opposites of those included in the embryonic list: (14) The facial region of the skull is large as compared with the cerebral; (15) the forehead is not prominent; (16) the superciliary ridges are more prominent; (17) the edges of the jaws are more prominent. Four characters, all of the head and face. It is thus evident that in attaining maturity man resembles more and more the apes in some important parts of his facial expression.

It must be noted here that the difference between the young and embryonic monkeys and the adults is quite the

Fig. 4.—Portrait of the same at seventeen years, showing the elongation of the facial region, and less protuberance of the cerebral.

same as those just mentioned as distinguishing the young from the adult of man (Figs. 1, 2). The change, however, in the case of the monkeys is greater than in the case of man. That is, in the monkeys the jaws and superciliary ridges become still more prominent than in man. As these characters result from a longer course of growth from the infant, it is evident that in these respects the apes are more fully developed than man. Man stops short in the development of the face, and is in so far more embryonic.* The prominent forehead and reduced jaws of man are characters of "retardation." The characters of the prominent nose, with its elevated bridge, is a result of "acceleration," since it is a superaddition to the quadrumanous type from both the standpoints both of palæontology and embryology.* The development of the bridge of the nose is no doubt directly connected with the development of the front of the cerebral part of the skull and ethnoid bone, which sooner or later carries the nasal bones with it.

(To be continued.)

* This fact has been well stated by C. S. Minot in the *Naturalist* for 1882, p. 511.

* See Cope, The Hypothesis of Evolution, New Haven, 1870, p. 31.

THE MORALITY OF HAPPINESS.

By Thomas Foster.

(*Continued from page* 106.)

THE EVOLUTION OF CONDUCT.

Chapter II.

IN considering the evolution of structures and functions we have not only to consider the influence of the struggle for individual existence, but also the effects of the contest in which each race as a whole is engaged,—and to do this we have to consider, first, those circumstances which affect the propagation of the race, secondly the relation of the individuals of the race to their fellows, thirdly the relations of the race as a whole to other races. Something akin to this must be done in considering the evolution of conduct. We have seen how modes of conduct which favour the continued existence of the individual are developed at the expense of modes of conduct having an opposite tendency. These last die out, because the individuals of the race who act in these ways die out. But it is obvious that conduct will be equally apt to die out which tends to prevent or limit the adequate renewal of the race from generation to generation. It is equally obvious that whatever conduct causes contests (whether for life or subsistence) within the race or species, tends to the elimination of members of the race, and so diminishes the chances of the race in the struggle for existence with other races. Lastly the relations of a race to surrounding races are manifestly of importance in the evolution of conduct, seeing that conduct will equally tend to be diminished whether it is unfavourable to the existence of the race in which it is prevalent, or simply unfavourable to the separate existence of an individual member of the race.

Now with regard to conduct affecting the propagation of a race, we find that, like conduct affecting individual life, it has been developed from what can hardly be called conduct at all in the lowest grades of life to fully developed conduct, with elaborate adaptation of means to ends, in the highest. In the lowest forms of life, propagation proceeds by mere division and subdivision, not depending so far as can be judged on any power of controlling the process, which such creatures may possess. In fact, the Protozoa multiply by dividing. We have to pass over many grades of life before we reach such imperfect care for propagation of the race as we find among those orders of fish in which the male keeps watch and ward over the eggs. Still higher must we pass before we find any trace of affection for the young, and higher yet before we see care given to feed and protect and keep the young till they are able to provide for themselves.

This brings us in fact very near to the human race, which, in its lowest races, is distinguished from other animals chiefly by the length of time during which it feeds, protects, and trains its young. In the higher human races all these processes are conducted with greater care and elaboration; more varied wants are considered and attended to, more elaborately varied means are used for the purpose. It is easily seen how such conduct by aiding the development of the race aids the development of the conduct itself by which that result is favoured. Among those members of a race in whom the proper race-propagating conduct is not adequately shown, propagation proceeds less effectively,—which is the same as saying that, relatively, such conduct itself must be diminishing.

This conclusion is not inconsistent, as at first sight it might appear, with the fact that mere numerical increase of propagation, though it means increase in quantity of life, is not always or even generally a proof of the growth of the race in what may be called race-vitality. Here as elsewhere adaptation of means to ends has to be considered, and that kind of conduct by which such adaptation is secured has the best chances of development in the long run. Let us, for instance, take an illustration from civilised life:—An early marriage between two persons careless alike of present duties and future difficulties, seems at first to tend directly to the increase of carelessness and thoughtlessness; for from such a union there will probably come into existence more than the average number of offspring, repeating in greater or less degree the weak characters of their parents: the totality of life characterised by undesirable qualities and conduct will thus be increased, and increased in a greater ratio than the totality of prudent, steady, and thoughtful life, by a well-considered union and well-judged conduct thereafter. Yet in the long run the result proves usually otherwise. (We consider only average results.) The larger number of offspring of inferior qualities, receive less care and inferior training: so that for them there is greater probability either of early death or of defective adult life. The parents suffer also in the struggle thus brought on them, for which they are ill-fitted. A diminished amount of life is likely to result, and (taking the average of many cases) probably does result; while certainly there is diminished life-quality. Hence results a correspondingly diminished amount and influence of the inferior kind of conduct shown by thoughtlessness or carelessness about life's duties. On the other hand, the well-judged and not too hasty union of two caretaking persons, though it may add a smaller number of individual lives to the life of the race, adds better and more enduring life, life more likely to maintain and sustain the qualities of the parents, giving therefore to these qualities in the race at once more stability and wider influence. In other words, the qualities best suited for the propagation of the race, and best suited for the race, will on the average be developed, while qualities having opposite tendencies will either be eliminated, or though they may remain will occupy a lower place and have diminished influence on the fortunes of the race—a circumstance tending of itself still further to their eventual elimination.

(*To be continued.*)

Pure nickel, after melting and casting, generally holds a greater or less quantity of oxygen in combination, and the metal is brittle. To hinder the injurious effects of the oxygen, it is necessary to incorporate in the melted nickel some substance which has a strong affinity for oxygen, and also for the nickel itself. According to the "Comptes Rendus," M. J. Garnier finds that phosphorus serves both of these purposes very satisfactorily, producing effects analogous to those of carbon in iron. If the phosphorus does not exceed three-tenths of one per cent., the nickel is soft and very malleable; above this quantity the hardness increases at the expense of the malleability. Phosphorised nickel, when alloyed with copper, zinc, or iron, gives results which are far superior to those that are obtained from the same nickel when not phosphorised. By means of the phosphorus Garnier has been able to alloy nickel and iron in all proportions, and always to obtain soft and malleable products. The contradictions of illustrious chemists are thus explained, some saying that such alloys were brittle, others that they were malleable. The latter had alloyed the nickel to phosphorised iron.—*Engineer.*

PRETTY PROOFS OF THE EARTH'S ROTUNDITY.

CHIEFLY FOR THE SEASIDE.

By Richard A. Proctor.

(*Continued from page* 117.)

BUT now, returning to the mirror proof of the earth's rotundity, some readers of these papers, comparing together Fig. 8 and Fig. 9 may be led to ask why, in looking into a mirror as in Fig. 9, we do not recognise the water surface *c d* as curved like the surface A B. Ought it not they may say to curve downwards on either side of the vertical centre line across the sweetly pretty face in Fig. 9, just as it does on one side of the point A? A little consideration will show that this is precisely the same difficulty which is described in the Paradox Column at p. 77 by the anti-Copernican carpenter Mr. Hardy. He thinks or rather is certain that if the curvature of the earth can be recognised in the direction of the line of sight it ought also to be recognisable at right angles to that direction. If a ship twenty miles away is hull down, owing to the earth's rotundity, a range of twenty miles of sea horizon ought to show well-marked convexity. If our pretty observer at p. 101 can really see the sea-horizon depressed owing to the rotundity of the earth as illustrated in Fig. 8, he ought to see the horizon curve downwards to right and left of him.

It may be worth while to mention at the same time another difficulty. In Fig. 8, as in Fig. 3 p. 69, and Fig. 5 p. 85 we have an upright on the right which is not really upright, but parallel to the true upright at A; would not the argument be altered, and especially the part of the proof which is geometrically given, were the uprights C *c* in Fig. 3 and Fig. 5 and B *b* in Fig. 8, made really square to the surface on which they are supposed to stand?

Taking the latter difficulty first, let it be noted that C *c* in Fig. 3 may be set vertical without in the slightest degree affecting the argument. In the argument at p. 85, C *c* is described only as approximately parallel to A *a*; while B *b* in the argument at p. 101 is spoken of as "appreciably parallel." In reality the upright B *b* in Fig. 8, the case where the departure from parallelism is greatest, is so nearly parallel to A *a*, that though the eye might just be able to recognise the departure from parallelism, the real difference between A *b'* and *a b* would be far too small to be noticed on the scale of the figure. The curvature of the surface A *m* B is in reality monstrously exaggerated—though necessarily; and A *a* which is supposed to represent a height of only 200 ft. really represents a height of many miles. There had to be exaggeration somewhere, or *b b'* and *b'* B would have been invisible; putting B *b* at right angles to the surface at B would not have made matters much worse, but it would have suggested the additional error that *b* is much farther from *a* than *b'* is from A. The argument would have been in no way affected.

Let us, however, inquire what the true proportions of the lines in Fig. 8 should have been, or the true lengths if any given length is assigned. Suppose *a b* to represent 17 miles, being in actual length 3·8 inches. Then since B *b* should represent 400 feet, and 400 feet are contained about 225 times in 17 miles, the length of B *b* in the figure should be the 225th part of 3·8 inches, or considerably less than the fiftieth of an inch. If the student will draw a line as A *b'*, 3·8 inches long, take A *a*, *b' b*, and *b'* B, each less than the hundredth of an inch, and fill in a curved surface A B touching A *b'* at A and *a* B at B, he will get a good idea at once of the slightness of the real curvature of A B and of the utter insignificance of any difference between the uprightness of the tiny line B *b* and the parallelism of that line to A *a*.

Or we may take B *b* the same in length as in Fig. 8 to represent 400 feet, and inquire what should be the length of *a b* to represent 17 miles. The actual length of B *b* is about seven-tenths of an inch. Multiply this by 225 and we get 157½ inches or more than 13 feet. Imagine B *b* and A *a* unaltered in length but set close on 4½ *yards* apart, A *b'* and *a* B drawn, and A *m* B carried with a circular sweep to touch these lines at A and B respectively. It would require a radius of about 1,000 yards or considerably more than half-a-mile. This is the kind of curvature which some think we ought to recognise in looking (as in Fig. 9) into a mirror half-a-foot, perhaps, square. As for the displacement of *b*, owing to B *b* being parallel to A *a* instead of perpendicular to the surface at *b*, this displacement even on the enormous scale just considered (in which B *b* represents 400 feet, and the radius of A *m* B is more than half-a-mile long) corresponds only to setting *b* to the left of its true place by less than the 200th part of the length B *b*, or by about 1-320th part of an inch.

Take we now the other difficulty, though it has indeed been already disposed of by the reasoning just run through.

Why should not the sea-horizon, whether viewed directly or by reflection in a mirror, seem curved?

Suppose an observer whose eye is 200 ft. above the sea-level looks at the long horizontal roof-ridge of a house, beyond which lies a sea horizon, and that he brings the middle of the ridge just below the sea-horizon exactly in front of him—ought he not as his eye ranges to right and left along the ridge, to lose the sea-horizon through its curving down below the ridge? Or, if he brought the ends of the ridge exactly level with the sea-horizon, ought not the sea-line to stand visibly above the middle of the ridge?

Theoretically it ought and it does; practically the question is one of degree, and our inquiry must be, how *much* does it curve?

Suppose the ridge to be 50 ft. long, and its middle point 25 feet from the eye, so that in sweeping along the ridge the eye ranges over a right angle. Suppose also a fixed point set near the eye to guide it, for otherwise the observation would be altogether inexact. Every line of sight must be taken athwart this fixed point, 25 feet from the centre of the perfectly horizontal ridge line, to different parts of this line; and what we want to find is how much the lines of sight to either end of the ridge line pass above a line of sight to the sea-horizon there, when a line of sight to the middle of the ridge line just touches the sea-horizon.

Let *a*, Fig. 13, be the fixed point athwart which the lines of sight are taken, E B F the roof ridge, 50 feet long, B its middle point; *a* B square to E F; and *a* B = E B = B F = 25 ft. Let *a* vertical plane through E B F cut the true horizontal plane through *a* in *e b f*, and let E' B' F' represent the sea-horizon as supposed to be seen on this plane; *e* E E', *b* B, and *f* F F' being vertical lines. We want to find the length of E E' and F F'.

Now obviously B *a b* in Fig. 13 represents the same angle as B *a b* in Fig. 8 as dealt with at p. 101. For, produced far enough—really to some 17⅓ miles—*a* B would meet the sea surface and be the *a* B of Fig. 8; *a b* of Fig. 13 would then be the *a b* of Fig. 8; and *b* B would therefore be 400 ft. The proportions of the triangle B *a b* would be precisely the same in both cases (because we are dealing with a point *a* in each case 200 ft. above the sea-level). In each case B *b* (as shown at p. 101) is 1-228th part of *a b*. But *a b* in Fig. 13 represents 25 ft. Therefore B *b* represents 25 ft. ÷ 228, or about 1⅓ in.

But it is clear that the triangles $e\,a\,E'$ and $f\,a\,F'$ are also similarly proportioned to $B\,a\,b$ of Fig. 8. For $a\,e$, like $a\,b$, is truly horizontal, and $a\,E'$ like $a\,B$ is directed to the sea-horizon. It matters not in what direction we look

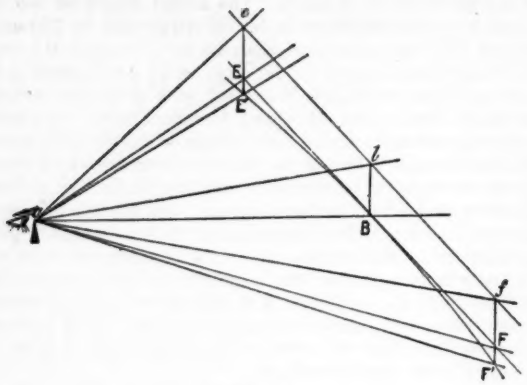

Fig. 13.

seawards from a fixed point above the sea; the sea-horizon has always from such a point the same depression. The only difference is that $a\,e$ and $a\,f$ are longer than $a\,b$, while $e\,E'$ and $f\,F'$ are longer than $b\,B$ in the same proportion.

Whereas then $B\,b$ is about $1\frac{1}{3}$ in. in length, $e\,E'$ and $f\,F'$ exceed $1\frac{1}{3}$ in. in the same degree that $a\,e$ or $a\,f$ exceeds $a\,b$; which is (appreciably) the same degree in which $a\,E$ or $a\,F$ exceeds $a\,B$, that is as the diagonal exceeds the side of a square (for $a\,B$ and $B\,E$ are equal and at right angles to each other). Thus since the diagonal of a square is about 1·414 when the side is 1, we have

$$e\,E' = f\,F' = 1\tfrac{1}{3} \text{ in.} \times 1{\cdot}414$$
and $e\,E = f\,F = 1\frac{1}{3}$ in. (each being equal to $B\,b$);
$$\therefore\ E\,E' = F\,F' = 1\tfrac{1}{3} \text{ in.} \times 0{\cdot}414 = {\cdot}55 \text{ in. approximately}$$

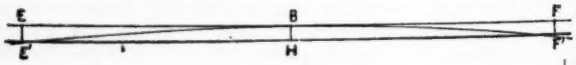

Fig. 14.

In reality, taking refraction into account the angles of depression are all reduced by about one-fifth, leaving $E\,E'$ and $F\,F'$ optically equal to only ·44 in., or $\frac{4}{9}$ths of an inch. The actual curvature of $E'\,B\,F'$ would be fairly shown if in such a diagram as Fig. 14, $E\,B$ and $B\,F$ were each 25 ft. long, $E\,E'$, $B\,H$, and $F\,F'$ each $\frac{4}{9}$ths of an inch long; and if then the curve $E\,B\,F$ were swept out in the narrow rectangle $E\,F'$, whose length *would be more than thirteen hundred times its breadth.*

So that even from so great a height as 20 ft., a ridge roof so long as 50 ft., seen from a distance of 25 ft., commanding therefore a range of a full right angle along the roof, and brought at the two extremities to exact apparent coincidence with the sea-horizon would be less than half an inch below the sea-horizon at its middle point, even if use were made of such a point as a (Fig. 13) to guide the eye.

But all the observations ever made in this way, as by Parallax in 1864, by Mr. Hardy (see page 77), and by a few perplexed students of this matter, have been made from lower levels than 200 ft., on ridges commanding a much smaller angle than a right angle, and without any such a point as a to guide the eye, though such a point is absolutely essential to exactness of observation. As for the mirror experiment of page 101, that, with a one-foot mirror and two yards distance would command an arc of less than five degrees of sea-horizon, and it would be preposterous to expect the slightest trace of curvature along such an arc.

(To be continued.)

PUNCTUATION AND PRINTERS.

I WONDER what the *Brighton Herald* takes to be "the rights and privileges of the humble comma." Apparently the right of distraction; for everything that unduly stops the continuity of thought in reading is distraction—parentheses, whether written so or not, unnecessary or too strong stops, all sorts of carts before horses, exceptions and conditions stated at the beginning instead of the end of a sentence, verbs before their nominative cases or after their accusatives, except for special reasons, anything that obliges you to wait till the end in order to realise what the sentence is about. These things, and not the mere length of sentences are the impediments to easy reading.

I forget whether you noticed before the printers' passion for *dibbling* in a comma between every two adjectives not already separated or joined by an *and*, without the least regard to its effect. For instance somebody might write that I am a "good tall man," meaning only that I am above the average height: the average printer would forthwith exalt me into a "good, tall man"; and thereby would raise the indignation of every architect in England into a still hotter flame than it is in just now I see, at my having dared to rebuild part of a cathedral without paying them some £1,500 of black-mail for doing worse than nothing, or something like the Law Courts.

But I want to go beyond commas, and to give even the Devil his due. Some men deliberately leave their punctuation to that inky Diabolus, and it is well for their readers and their own credit that they do. Within the last month or so I have had letters from two men of excellent education, both High Wranglers and men of science—one old and the other young—whose only stops were commas and dashes, dashed in pretty much *ad libitum*. Such writing would justify quite natural language from a printer's devil; and if he is to be condemned himself for doing his best—and rather overdoing it—he may fairly call the world ungrateful. But, on the other hand, he must see in five minutes whether he is printing for a punctual punctuator like you or me, who, at any rate, mean to put in all our stops; and when he sees that he ought to leave us alone to bear our own iniquities if we let our proofs go "unpeppered" more than he thinks right. We probably know better than he does.

The fact is that any one who attempts either to punctuate or to write English entirely by rules is sure to disregard both sense and common sense sometimes. But one rule may be safely followed, viz., the advice of some judicious bishop to his clergy on the length of sermons, to "err on the side of leniency;" and the other still older rule—*Quid dubitas ne feceris*, if you doubt about a comma leave it out.

The Greeks were wiser than we are in not encumbering themselves with both colon and semicolon. Nobody can say with certainty *ὅσῳ πλέον ἥμισυ πάντος*, how much less a semicolon is than "the entire animal," from which it differs only by a tail. What is really wanted is a kind of minor comma, or both that and the semicolon reducing a little in power of obstruction. But that reform is as hopeless as an Easter by the sun instead of the (Act of

Parliament) moon, though everybody would welcome it if it came somehow with no questions asked.

If anybody doubts your opinion of Macaulay's style, which was always mine, let them try it by the test of reading it aloud, against Froude's, or Hume's old but matchless History, and they will soon find how tiresome it is. I have made his admirers give in by that test. Gibbons is sickening when you have learnt the trick of it, and its want of simplicity. I never saw Mr. Spencer's "Essay on Style" (is it a separate book?*). Some of his own is good enough; but when he wants to be particularly precise and lucid he is often particularly the contrary, and his style, in my opinion, most abominable. Nevertheless, he may preach better than he practises, and inculcate better English than philosophy, in spite of Mr. T. Foster.

EDM. BECKETT.

THE FACE OF THE SKY.

FROM AUG. 31 TO SEPT. 14.

BY F.R.A.S.

THE usual daily watch will be kept upon the sun for spots and faculæ. The aspect of the night sky is shown in Map IX. of "The Stars in their Seasons." Mercury attains his greatest eastern elongation (26° 42′) from the sun on the 11th, but sets too nearly with the sun now to be visible to the naked eye. Venus is quite invisible. Mars rises before 11 h. 30 m. p.m. now; but, as we said a fortnight ago, merely presents the appearance of a very large red star in Gemini. Jupiter does not yet rise until after midnight. Saturn rises before 10 p.m. on the 1st of September, and soon after 9 o'clock by the 14th, so that he will be well above the horizon by midnight. He is situated between 3° and 4° north of Aldebaran, and just to the East of it. Uranus and Neptune are both invisible. The moon's age at noon on Aug. 31 is 28·4 days; and at the same time to-morrow it will evidently be 29·4 days. Her age on Sept. 2, at noon, is 0·9 day, and quite obviously will be 12·9 days by the 14th. Two occultations of stars will take place on the night of the 14th—firstly of c^1 Capricorni, a 4½ magnitude star, which will disappear at the moon's dark limb at 8 h. 44 m. at an angle of 171° from her vertex, reappearing at her bright limb, at an angle of 208° from her vertex, at 9 h. 5m. p.m. At 8 h. 47 m. p.m. c^2 Capricorni, a star of the 6th magnitude, will disappear at the dark limb of the moon, at a vertical angle of 91°, to reappear at her bright limb, at 10 h. p.m. at an angle of 300° from her vertex. The moon, after travelling through a portion of Leo, descends into Sextans about 10 p.m. on Aug. 31, and (occupying about 9 hours in crossing the northern part of that constellation) re-enters Leo at between 7 and 8 p.m. on Sept. 1. It is between 9 and 10 o'clock on the night of Sept. 2 before she finally quits Leo and enters Virgo. It takes her until 9 a.m. on the 6th to cross this constellation; from which, at the hour named, she passes into Libra. Her passage through Libra occupies, as nearly as may be, 48 hours, and about 9 a.m. on the 8th she enters the northern part of Scorpio; over this she takes 12 hours to travel, and at 9 p.m. crosses into the southern part of Ophiuchus. Skirting, for a very short time, the extreme southern limit of Serpens, she travels into Sagittarius about 5 o'clock in the afternoon of the 10th. She does not leave this constellation for Capricornus until 2 a.m. on the 13th. It takes her until between 5 and 6 p.m. (or some 15½ hours) to go over the northern part of Capricornus, which she then quits for Aquarius. She is still crossing Aquarius on the 14th.

IT is stated that there are 3,985 paper-mills in the world, producing yearly 959,000 tons of paper made from all kinds of substances, including rags, straw, and alfa. About one-half the quantity is printed upon; and of those 476,000 tons, about 300,000 tons are used by newspapers. The various Governments consume in official business about 100,000 tons; schools, 90,000 tons; commerce, 120,000 tons; industry, 90,000 tons; and private correspondence another 90,000 tons. The paper trade employs 192,000 hands, including women and children.

* It is in the second volume of his collected essays.—R. P.

"Let Knowledge grow from more to more."—ALFRED TENNYSON.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

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PHENOMENA OBSERVED IN RAILWAY TRAVELLING.

[906]—When travelling in a train and looking out of a window, the nearest objects appear to me, of course, to fly rapidly backwards, and the more distant to go slower until those on the horizon seem to travel slowly in the same direction as the train. If I have been looking out of the window at the landscape for some time, and the train stops, the landscape appears to slowly unwind itself, as it were, the nearer objects moving slowly forwards, and the remoter just perceptibly backwards. I should like to know whether this is a common experience, or one peculiar to my own somewhat over-sensitive nerves. I should also like to know whether the phenomenon is analogous to that of complementary colours. I explain it to myself by supposing that while the train is in motion the nerves of the lower part of the retina are continually conveying impressions of forward motion (the nearer objects being, of course, reflected upon the lower part of the retina), and those of the upper part conveying impressions of backward motion. Then, when the nerves are no longer excited from the outside they rest themselves by complementary sensations, as in the case of complementary colours.

Hoping that I have not wasted your valuable time in describing what you are already familiar with, LEONARD BROWN.

LUMINOUS RING.

[907]—I enclose two photographic prints—one of my chemical laboratory, the other of my dining-room—both taken by gaslight. In each of these (and, of course, in the negatives) I observe a perfectly well-defined circle of light surrounding the gas-flame.

I showed the pictures to my friend Mr. Grensted, who gave what seems to me a very satisfactory explanation of the matter, and one, indeed, which is interesting as affording another instance of the analogy which exists between the human eye and the photographic camera. CHAS. HARRIS.

With regard to the above, I have for many years seen a prismatically-coloured similar ring, with the naked eye, when looking at a candle-flame against a background of shadow. I thought it to be either an emanation from the flame or caused by floating particles in the eye. The first hypothesis is disproved by the fact that one of the prints shows the ring overlapping the frame in which it is reflected. The constitution of a camera lens disproves the second. Is it a diffraction ring caused by reflections from the interior surfaces of the lens? FRED. F. GRENSTED.

FLIGHT OF A VERTICAL MISSILE.

[908]—*Conservation of Angular Momentum.* By this principle practical engineers perform such operations as adjusting a balance-weight to the driving-wheel of a locomotive. Stated in this form it carries its own proof with it, being simply another expression for the law that action and reaction are equal and opposite. Had the principle been generally known by this phrase, it would have guided your correspondent, Mr. Bray, to a correct solution of the problem. The ball, instead of moving east with the velocity of a point on the earth's surface, moves east with a velocity inversely as its distance from the earth's centre. Measuring the arc so described at the mean height evidently doubles his former result.

XUM

The term "Kepler's second law" conveys no particular meaning—in fact, Clifford, in his "Dynamic," 1878, calls it Kepler's first law, and it is sometimes misunderstood to apply only to forces varying inversely as the square of the distance. "Angular momentum" seems a preferable term to "moment of momentum." "Conservation of areas" is a very misleading designation.

The writer suggests that in the same way as the term *half the vis viva* has been superseded by *energy*, so now might *Kepler's second law* be replaced with advantage by the expression *Conservation of angular momentum*.

A. Barclay.

WART-CHARMING (SO-CALLED).

[909].—I am both amazed and amused at the wonderful theories started upon the very simple subject of destroying warts. Will you allow me to give a simple explanation of a natural process, which has as little connection with the "imagination" or the "influence of mind on body" as the moon has with green cheese?

Pasteur has killed small animals by inoculating them with the saliva of an ordinary living person. To this poisonous quality of the saliva (especially so in the mornings and evenings) most of the feats of so-called wart-charming may no doubt be traced.

I myself had a wart on my left cheek for a very considerable time, caused, no doubt, by an unclean razor. I wetted the wart with my own saliva every morning and evening, and in about a fortnight the wart had entirely disappeared.

It is evident that persons less fond of soap and water, or those with disordered stomachs, possessing, therefore, a more poisonous saliva, may effect quicker cures.

I have always found it more reliable and satisfactory to seek for the natural cause of everything than to ascribe it to the supernatural and marvellous.

Mephisto.

[Our friend from warmer regions ought to know what is supernatural and what not; but he appears not to do so. The influence of mind on body is among observed natural phenomena, and cases of so-called wart-charming are among the most interesting illustrations, but certainly not by any means the most surprising. The saliva is not used in one case out of twenty.—R. P.]

SOME AMERICAN WARTS.

[910]—When a boy in Concord, Massachusetts, only thirteen years ago, I had about a dozen warts on my hands. Another boy told me that Mr. C——, the jeweller, could charm them away. I had two years before removed a lot of warts under direction of my mother by applying three times a day a saturated solution of sodic carbonate, and had much less faith in charms than in chemicals. My playmate, however, maintained that his warts had been removed by the charming process, and so I went to the jeweller and asked the same favour. He rubbed his fingers over each one, and told me to put on them every day the juice of the milkweed (*Asclepias cornuti*). I did this as often as I thought of it, and the warts gradually became smaller, and after a few weeks I was surprised to notice that they were gone. The success did not convince me of the efficacy of charming, for "they say" in Massachusetts that milkweed juice will cure warts, and I have thought that the metals which the jeweller handled may have given some virtue to his touch.

Fritz F.

New York, *Aug.* 7.

GINGER-BEER PLANT.

[911]—The ginger-beer plant described by T. H. Perry (878) is probably an aggregation of one of the yeast-plants. The aëration of the water is no doubt effected by the vegetative process induced by the sugar. Substitute essence of lemon for the ginger, and you will have a "lemonade" plant.

W. H. Shrubsole.

IRON SHIPS AND FLOATING DOCKS.

[912]—Will you kindly allow me to inquire through your columns whether the builders of iron ships ever make allowance for the effects of wide differences of temperature in producing irregular expansion and contraction of the metal, and consequent cracks and fissures? Fownes says in his "Manual of Chemistry" that "the force exerted in the act of expansion (by heat) is very great. In laying down railways, building iron bridges, erecting long ranges of steam-pipes, and in executing all works of the kind in which metal is largely used, it is indispensable to make provision for these changes of dimensions." Yet I am told that this rule does not apply to iron ships, as in them the expansion or contraction is uniform. May I venture to ask if the immersion in water has no neutralising effect, and whether also the building of iron ships has been tried to any extent in countries subject to great variations of temperature, such as Norway and Northern Russia? To put the question as plainly as I can in my utter ignorance of the appropriate technical language, would a builder be willing to leave the shell of an iron ship the whole year through without any covering in such a country as those just named, and expect no harm to result from the vicissitudes of the weather, leaving out of view the liability to rust?

I am induced thus to trouble you on account of a question which has lately arisen as to the immunity of iron floating docks from the risk of having their efficiency impaired by exposure to the wide variations of temperature that occur in certain climates. It has been stated positively by a presumed authority that "the expansion and contraction of the iron in a dock would have the same effect as in a ship," and yet it seems to me that the resemblance between the two structures is not so exact as to make that proposition self-evident.

Again, it has been publicly stated that "as the advanced science of the present day demands the use of iron and steel for the construction of ships, so it does for docks, and in the whole of Europe at the present time no engineer would think of using any other material in their construction." Is this true or is it not?

X + Y.

HEIGHT OF ANCIENT EGYPTIANS.

[913]—Being much interested in Egyptology in all its phases, I recently made a tour up the Nile, and having previously noted some statements respecting the sizes of certain mummies, when in Cairo I measured several, especially those of the recent great find in Thebes. I give my measurements below, and would be glad if it leads to an "official" publication, as it would settle a good many points in reference to this interesting and remarkable ancient race.

Measurements.	
King Unas VI. Dynasty	4 ft. 9 in.
King Ra Skemen XVII. Dynasty	5 „ 8 „
King Amenoph 1st XVIII. Dynasty	5 „ 2 „
King Amosis XVIII. Dynasty	5 „ 0 „
King Thothmes 2nd XVIII. Dynasty	5 „ 6 „
King Sethi 1st XIX. Dynasty	5 „ 2 „
King Rameses 2nd XIX. Dynasty	5 „ 1 „
King Pinotem 1st XXI. Dynasty	5 „ 0 „
Queen Nofritasi XVIII. Dynasty	5 „ 4 „
Queen Notimaut XIX. Dynasty	4 „ 8 „
Queen Makara XXI. Dynasty	4 „ 6 „
Baby of ditto	1 „ 0 „
Princess Trinkheb XXI. Dynasty	4 „ 9 „
High Priest Nebsemui XXI. Dynasty	5 „ 6 „

The coffin of Rameses measures only 6 ft. 3 in. over all, and the mummy does not reach to the wood (the thickness of which is 2 in.) by several inches.

Wm. Oxley.

SINGULAR NUMERICAL PROPERTY.

[914]—I send the following as curious example of figure-magic, with the hope that it may be of interest to readers of Knowledge:—

If the number 142,857 be multiplied by 1, 2, 3, 4, 5, or 6, each result gives the same figures in the same order, only beginning at a different point. Thus:—

$$142{,}857 \times 1 = 142{,}857$$
$$142{,}857 \times 2 = 285{,}714$$
$$142{,}857 \times 3 = 428{,}571$$
$$142{,}857 \times 4 = 571{,}428$$
$$142{,}857 \times 5 = 714{,}285$$
$$142{,}857 \times 6 = 857{,}142$$

If it be multiplied by 7, we get all nines. Thus:—

$$142{,}857 \times 7 = 999{,}999$$

If it be multiplied by 8, we get 1,142,856. Now add the first figure to the last, and we have the original number—142,857. If it be multiplied by 9, we get 1,285,713, and then we find that the sum of the digits of the product is equal to the sum of the digits of the original number, each being 27.

Moreover, the sum of the digits of each product is the same in every case but one, namely, when we multiply by 7, and then the sum of the digits is 54, or just double the sum of the digits in each of the other products.

H. Askew.

[The number 142,857 is 999,999÷7, and is also what we get on dividing 1,000,000 by 7, less ⅐. Now noting that 10, 20, 30, 40, 50, or 60, when divided by 7 leave remainders 3, 6, 2, 5, 1, and 4, &c., all the digits less than 7, that 80 divided by 7 gives 11, with remainder 3, and that 90 divided by 7 gives 12 and remainder 6, we find all the above facts accounted for, seeing that the digits in dividing 1000000, 2000000, 3000000, &c., to 6000000 must recur in

the same order, though beginning with a different digit of the set; in the case of 8000000 and 9000000 we have $999,999 \times 8 + 1\frac{1}{7}$, and $999,999 \times 9 + 1\frac{2}{7}$.—R. P.]

LETTERS RECEIVED, AND SHORT ANSWERS.

W. Bayley.—Thanks for cuttings; but of course the toads were never vomited.—J. Clayton. Handbook of stars out of print.—Jas. Paxman. Illness following railway accident drove the matter from my thoughts.—J. Harrison, sen. Many thanks for the weather charts.—A Reader. You will have seen ere this that I quite agree with your objections to cholera-prescribing here, as in *Health*, and other such organs.—Ignoramus. Stephenson said to a perpetual motionist, "Carry yourself round the room by your own waist-band, and I will consider your plan." That is about what it comes to.—W. W. W. Regret, but no space.—Jas. Lucking. Do not know where model of first pair of spectacles can be seen. Am also unable to say where a photograph of Adam and Eve is preserved.—A. H. Swinton.—Your determination of sun-spot maxima from years of great natural phenomena seems to me as funny as determining comets—otherwise unknown—from deaths of kings and rulers. Prove the connection between volcanoes and sun-spots and we will see about it.—H. H. Thanks for kind wishes.—D. N. Certainly not new. A hundred such relations could be written down in a few hours.—E. C. Castlebar. Earth's shape seems to trouble you. When and where have I said I admired the person you mention? But reading the sickly works of the other writer would have had no influence one way or the other.—E. H. Stutter. Many thanks for your kind letter.—Senex. Not unusual. Showing want of symmetry, not that penumbra was not lower than photosphere.—H. W. Jones. No space or time for explanations of so much text-book matter.

Our Mathematical Column.

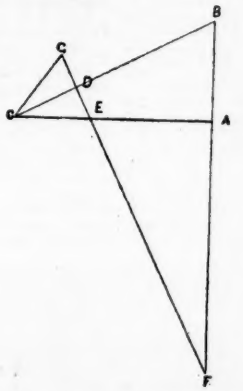
GEOMETRICAL PROBLEMS.

By Richard A. Proctor.

PART XIV.

A READER of these mathematical notes sends for solution, by use only of Euclid's first three books, the following problem:—

Prob.—*A B C is a right-angled triangle. From any point* D *in the hypothenuse* B C *a straight line is drawn at right angles to* B C, *meeting* C A *at* E *and* B A *produced at* F: *show that the square in* D E *is equal to the difference of the rectangles* B D, D C *and* A E, E C; *and that the square on* D F *is equal to the sum of the rectangles* B D, D C *and* A F, F B.



We notice at once, that the rectangle A E, E C mentioned in first part of the problem is equal to the rectangle D E, E F, since the angles at D and A are right angles so that a circle will pass through the points C, D, A, F. Also we notice at once that the rectangle A F, F B mentioned in the second part of the problem is equal to the rectangle E F, F D, since a circle will pass through the points A, B, D, E. This leads us at once to think that we may find the solution of our problem, by using those theorems of the second book of Euclid in which a square on D E, a part of a line, is shown to be the difference of two rectangles, and a square on D F, the whole of a divided line, is shown to be the sum of two rectangles. But in each case we must bring in the rectangle related to the line D E F which we have seen to be equal to a rectangle referred to in the puzzle.

Thus, first, we ask how the rectangle D E, E F is related to the square on D E. We know that

$$DE^2 = \text{rect. } DE \,.\, DF - \text{rect. } DE \,.\, EF.$$

If then we can show that the rect. D E. D F is equal to the rectangle C D, D B, what is required, so far as first part of our problem is concerned, is done. Now as we are dealing with the third book, this naturally leads to the idea that if a circle is carried around the points C B F, we may be able to show that F D which intersects C B in D, will when produced cut this circle in G, such that D G = D E. For we want to show that the rect. C D, D B = rect. D F, D E; and we know that the rect. C D, D B = rect. F D, D G. But this leads us directly to the solution of this part of the problem. For if we suppose the circle through C, B, F, to cut F D produced in G, we have $\angle CGF = \angle CBF$ in same segment $= \angle CED$ (since each is the complement of $\angle BCA$): hence the triangles C G D and C E D are equal in all respects, and D E = D G.

The other part of the problem is found to depend on the same construction: for

$$DF^2 = DF \,.\, EF + DF \,.\, DE = BF \,.\, AF + DF \,.\, DG$$
$$= BF \,.\, AF + CD \,.\, DB$$

We may put our solution into the following form:—

Produce F D to G making D G = D E and join G C. Then since the angles at D are right angles

$\angle CGD = \angle DEC =$ compt. of $\angle DCE = \angle CBA$.

Hence a circle will pass through the points C, G, B, F; also obviously a circle will pass through the points C, D, A, F; and another circle through the points D, E, A, B. Hence

rect. C D, D B = rect. G D, D F = rect. D F, D E
rect. C E, E A = rect. E F, D E
and rect. A F, F B = rect. D F, F E

$\therefore$ rect. C D, D B − rect. C E, E A = rect. D F, D E − rect. E F, D E = DE^2
and rect. CD, DB + rect. A F, F B = rect. D F, D E + rect. DF, FE = DF^2

Q. E. D.

Our Whist Column.

By "Five of Clubs."

TAKING PARTNER'S TRICK.

MY partner leads a Queen original lead, I hold Ace, King, and Two. Not a possible trick in any other suit, save Trumps. Love all. To his lead, I play the King, lead the Two of Trumps, having Two, Three, Knave, and Queen. My partner wins with the King, leads Ace, follows with the Four, leaving me with the thirteenth trump and the lead; I play Ace of his suit, then the small one. Have I made the most of my hand? Did I lose anything by taking his Queen with my King? Are there any circumstances under which I can lose by playing King on my partner's Queen, holding Ace, King, and one small one? Supposing my partner knows the conventionalities of modern Whist, I expect him to play from his long suit. No card in my hand or played by second player tells me whether his lead is from weakness or strength; therefore I put on my King in order to get out of his way. If his lead is from two I assume he has not four trumps, and wishes to make a trump on that suit. If his lead is from Queen, Knave, and a small one, or Queen, Knave, and anything, I contend I lose nothing by playing my King. If he hold Queen and one other and I let it go, he plays the small one I put on my King. If I then play my Ace his discard is no use to him, or me either, and instead of playing to win the game I am forced to feel my way to the end. He has exposed his hand to the entire party, and leaves me to get out of the mess the best way I can. If he holds a long suit, of which he leads the Queen, I contend that to play the small card would be wrong. The play succeeded. His suit proved to be a long one, Queen, Knave, ten, and others. He afterwards told me I knew nothing about the game, and never to do so again, although it "came off." "He had played Whist forty years." I being a young player, gave up the argument, believing I was right. He also advised me never to take my partner's trick; but having brought off *Deschapelles' coup* five times last winter, I voted my old friend a "duffer."

Hoping I have not exhausted your patience, and that I may be informed in your next, I am, dear Sir, yours, &c. Muff.

[If you had played your small card, the evil consequences which would almost inevitably have followed would have emphatically justified the name you assume—without having made out any case for its use. Your partner only *thinks* he has played *Whist* forty years.—Five of Clubs.]

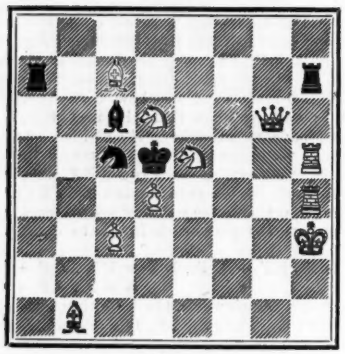
Our Chess Column.

By Mephisto.

PROBLEM No. 94.

By E. N. Frankenstein.

Black.



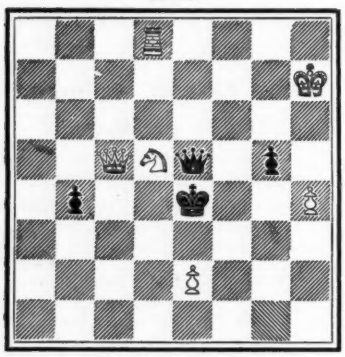
White.

White to play and self-mate in five moves.

PROBLEM No. 95.

By J. C. S.

Black.



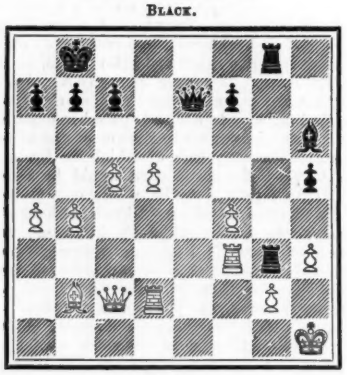
White.

White to play and mate in three moves.

ENDING FROM ACTUAL PLAY.

E. N. Frankenstein.

Black.



White.

Dr. P.

Black to play and win in the least number of moves.

SOLUTIONS.

Problem No. 91, by W. Terrill, p. 96.

We are glad to say that this problem is quite correct as published, which may be seen on working out the solution. The position is as follows:—White—K on K sq., R QR sq., P QKt2, R KR3, P KB4, Kt Q5, Kt QR5, Q QB6, B K6. Black—B or KR sq., Q KB sq., R KR4, P Q5, B QKt6, K Q6, P K6.

1. Kt to B3	R to Q4		P takes Kt
2. Q to B4 (ch)	B takes Q	B takes B	Q to Kt4
3. Castles mate.		R to Q sq., mate.	

(if 2. K to B7, 3. Q to Kt2, mate.

Problem No. 92, by J. C. S., p. 112.

1. B to B2	1. P to K6
2. R to B4	2. P takes B
3. P to K4, mate.	

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

W.—Castling is not prohibited in problems, although interdicted in some tournaments. Castling being a legitimate move at Chess, we fail to comprehend why it should not be resorted to. Clearly, if there is no other way of effecting a mate than by Castling, it is evidently then the author's idea, as suggested by the non possibility of effecting a mate in any other way. We do not see any trickery about it. If a composer was to construct a problem which admitted of a mate being given by Castling as well as by another move, and in reply to the solution by Castling were to declare that move impossible on account of the K having previously moved, that would amount to deception, and would be inadmissable. In no other way can Castling possibly lead to a misunderstanding. However, as long as the majority of problemists are opposed to Castling, it ought not to be resorted to for that reason.

W. Terrill.—Pray excuse our error; we have been led into it by a correspondent.

J. Berger Gratz.—Letter and contents received with many thanks.

Berrow.—If 1. P takes Kt, then 2. B takes B. See solution published above.

C. Planck.—Problem received with thanks.

William W. Thomson.—If 1. B takes Q, P takes B. 2. Q to R sq. (ch), the Kt can interpose on R4. See solution published above.

R. B. Sargeant.—We shall make inquiries and inform you accordingly.

Problem No. 91 correctly solved by Schmucke, H. A. L. S., and John; and Problem No. 93 by Stanley B. Baxter, M. T. Hooton, John Watson, John, Schmucke, Berrow, W. R. Edwards, and H. A. L. S.

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